M. Schwarz

26th December.

Dear Josh,

Very nice to hear from you, in your letter and Esther's note. It sounds as though things are flourishing. We also heard indirectly from Cecily Curry, now with Kaplan, that plenty is cooking in your scientific pot.

I could come for 6 months, starting at the end of June, any year. I would love to come this year, but assume that it is now probably too late to organise this. The Carnegie Fund for Scotland might well cover my travel and support for 3 months or so; but much the pleasantest thing would be to bring over the whole family. This would not be possible unless we could tap some U.S. source. Perhaps $ 5000 would cover travel plus living.

Your and Nossal's specific adherence phenomenon sound promising; I expect you know more about it by now. Hasnt something of the same sort been claimed for thoracic duct lymphocytes? Gowans is engaged on a search for a sort of immune adherence, but with non-sensitised cells. He takes an F1 rat, and drains out it's l'cytes; then injects enough l'cytes from one of the parental strain animals to kill it by a graft-vs-host reaction. Immediately after the injection, he drains out from the thoracic duct enough cell (oresumably of donor origin) to kill another F1 animal. But this time they wont kill, even though they can induce runt disease in a third-party new-born. This is only one experiment, and needs to be cleared up. But how nice if one can use a rat as a filter.

Boyse's work is in Immunology, this year. It's not numerically very convincing. Someone here tried to repeat it with Salmonella H antigen, without success. But I think it would be worth going into again. I've tried to break erythrocyte tolerance in chickens, by using leucocytes from the tolerant bird to do graft-vs-host reactions in third-party eggs, and then transplanting the enlarged spleen back into the tolerant birds; but with no success at all. Of course the control you suggest is required, and we included it in the mouse experiment.

Yes, I quite agree about the desirability of using autologous carrier protein, if one is working with haptens. My own efforts with autologous erythrocytes have failed so far, with cells diazotised to sulphonic acid and with cells reacted with DNBSO$_3$. But I'm going to carry on. The point is to find some group where there is a reasonable chance of follwoing the metabolism in tolerant animals.

Our main effort here this year has gone on the kinetics of tolerance: David Dresser with tolerance of mice to BGG, myself with tolerance of chickens to homologous and turkey erythrocytes. We've both been disturbed by the irregularity of tolerance, and by the transience of states of tolerance which seem complete and which are liberally fed with antigen. The only things I feel fairly sure of are (i) only complete tolerance is stable, (ii) the parameters of tolerance (e.g. the length of time without circulating antigen reqiired to break tolerance; or the quantity of antigen per unit body weight required for mantainance) change progressively until well past "immunological maturity", and (iii) a good deal of descriptive biology has to be cleared up before one can get down to the more interesting experiments on breaking tolerance.

As ever. Arno